MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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PROFESSOR CAYLEY, President, in the Chair.

The Rev. Thomas Tordiffe, Workington ;
The Rev. Wm. Falconer, Bushey ;
The Rev. F. W. Hall Courtier, Clare College, Cambridge ;
Edw. Sheppard, Esq., Winchester ;
W. M. Williams, Esq., Woodside, Croydon ; and
H. W. Jackson, Esq., Tooting,

were balloted for and duly elected Fellows of the Society.

Spectroscopic Observations of the Zodiacal Light, in April 1872, *at the Royal Observatory, Palermo.* By C. Piazzi-Smyth, Astronomer Royal for Scotland.

Introductory Account.

Having been required last winter to seek a warmer climate for a time, I endeavoured when away to utilize the opportunity by repeating those observations on the zodiacal light, in which I had completely failed in obtaining any positive result through the whole spring season of 1871, on the top of Calton Hill, Edinburgh. The failure there arose from the smokiness and gas-illumined atmosphere of a northern city, preventing so faint and low a light influence as the zodiacal manifestation is there, being ever seen or recognised as such.

But when fine weather broke at last upon me, on March 1, 1872, in latitude 40° 30′ N., far away from gas and smoke, I did expect to see something that evening, though I was hardly prepared for the full magnificence and vigour with which, from 8 to 9 P.M., local mean time, the well-known lenticular form of the zodiacal light was seen cutting obliquely up amongst the

stars, and contrasting notably with everything else visible in the sky at the same time. Thereby the remembrances of thirty years ago, in S. Lat. 33°, were instantly brought back to me ; the zodiacal light was still to my eye as it was then, several times brighter than the Milky Way, of regular mathematical figure and shading, and altogether one of the grand phenomena which an astronomer might worthily desire to probe the possible nature of. So also it continued to be seen, changing its place regularly among the stars with the Sun's motion, both in Right Ascension and Declination, through the months of March, April, and May, whenever absence of the Moon, twilight, and clouds, permitted a fair observation to be taken in any latitudes approaching to 38° or 40° north.

During the middle of the month of March, being stationary in Palermo for a time, I prepared my spectroscope, at the hotel window where I was staying, for acting on the zodiacal light as soon as the moonlight evenings should cease. But having in the meanwhile made the personal acquaintance of Signor Cacciatore, the Director, and Professor Tacchini, the chief observer in the Royal Observatory of Palermo as well as secretary to the Spectroscopic Society of Italy and editor of its transactions, they saw the apparatus on paying me a visit one day, and immediately proposed that I should institute all the intended observations at their Royal Observatory, whose elevated situation was far more appropriate for the purpose than any house in the city below. To this proposal, made in the most admirable spirit of fraternal accord in working for the promotion of knowledge, I was most happy to consent, and all the more because opposite conclusions in the problem were already in the field, and I wished no step in my process to be concealed or mysterious. So during the several days which must still intervene before the Moon should be completely eliminated from the early night sky, the apparatus was taken to the Palermitan Observatory, and tried occasionally on test-objects; while in frequent day visits I profited in solar spectroscopy by seeing Signor Tacchini's masterly handling of the 20-prismed instrument attached to the Palermitan equatoreal, comparing his daily drawings of red prominences with those furnished for the same dates by Padre Secchi at Rome, and Signor Lorenzoni at Padua ; and generally in reading up all that Italian spectroscope astronomers had been doing for some time past, both on the aurora and zodiacal light, as well as on the Sun, in the papers, both printed and MS., of " the Spectroscopic Society of Italy," freely lent to me.

At last the first favourable evening arrived, the spectroscope was already in suitable position for its cosmic work at the Observatory; and on April 1, from 7.45 to 8.50 P.M. the zodiacal light, admirably and characteristically visible, was well worked by Signor Cacciatore and myself ; again, on 3rd of April, during the same hours, by Signor Cacciatore, Signor Tacchini, and myself ; and again, on the 5th of April, by Signor Cacciatore,

Signor Ricca, my wife, and myself. Each evening gave the same results, and every observer saw the same spectrum in the same manner, excepting only in some minute and unimportant residual differences of different eyes when tried on an instrumental appearance of the very last degree of faintness. And the spectrum which was thus found for the zodiacal light was a short continuous spectrum extending from near to W. L. 5550 to near W. L. 5000, and culminating as to its brightness in or about W. L. 5350, while it was shaded off indefinitely into darkness at either end.

In short, a spectrum this was, as different from that of the aurora when seen in the same instrument, "as night from day," according to the description of a lady who had seen the spectra of both these phenomena in the same instrument, on different occasions, and was invited to give her opinion in the Palermitan Observatory on our concluding zodiacal light evening of April 5 —an opinion, too, in which I perfectly coincide ; but then I am required as a man to give a reason for the opinion that was in me, and to show that no other was possible to any good head or clear eye. I must crave, therefore, sufficient space to set forth,—

1st. A description of the Edinburgh faint-light spectroscope.

2nd. Comparison of the new results, with older ones already published ; and

3rd. Phosphorescence spectra, astronomical and terrestrial.

The Edinburgh Faint-light Spectroscope.

This instrument was built up by degrees at home in 1870 and 1871. I could make a better one now, and applied to the Royal Society before going out in February for a portion of the Government grant to construct such an apparatus specially for the zodiacal light ; but as the Council refused to give me anything, I had no resource but to continue the employment of the old instrument, intended mostly for the aurora, but capable, fortunately, of proving some points in observation which are generally rather uncertain at night, and particularly with portable instruments in unusual situations. Thus, say that an observer looks (as I did when at sea on March 1, 1872) at the zodiacal light with a little hand spectroscope casually purchased, and sees something or other excessively faint near the middle of a perfectly dark field, what account can he give of it ? The light is too weak to show any colour; there is nothing else visible in the darkness to fix the spectral place by, nor to prove the width of the slit, nor the focal sharpness of that slit, nor to show the amount of dispersion of the prism ; yet each and all of these points should be fixed there and then, or nothing certain has been arrived at.

The above observation-difficulty was met in the larger

Edinburgh apparatus by employing invariably a reference spectrum formed out of the blue part of the flame of an alcohol lamp, very slightly tinged with sodium and lithium chlorides, but with all the upper yellow or continuous-spectrum giving part of the flame cut off. This sort of light, then, being brought in sideways, and passed by reflection through the lower part of the same slit and prisms used in examining the cosmic phenomenon, gave its own known spectrum in the lower part of the field, and enabled the observer, though in perfect darkness, to know, first of all, whether he was looking straight into the prism eyepiece; second, to test the focus of slit by the sharpness of the edges of the lithium and sodium lines; third, the breadth of the whole field by the visibility of all the five bands of the blue alcohol light from red to violet; fourth, the width of slit by the width of the above lines compared with their distance asunder; and fifth, the dispersion power of the prisms by the degree to which, with the finest slit and the best focus at that part of the spectrum, either the citron or the green band of the blue flame could be separated into its component fine lines; such state of the slit and focus being found also to be the best for seeing the Fraunhofer black lines by day in the same spectral region.

Now in the Edinburgh apparatus, when consisting optically of a slit, an achromatic collimating lens adjustible for focus, and a direct-vision prism-combination about four inches long, I could with such a "Fraunhofer-line slit," directed on the reference-lamp, only fancy that I saw the separate lines: *i. e.* the resolution of the *bands* was not clear, though it had commenced.

With a second similar prism, or with a little opera-glass used in addition, the resolution of the bands was certain, and their lines were seen separated from each other by about twice their own thickness; but these additional aids to dispersion effects could never be used with advantage on the zodiacal light by reason of the peculiar faintness of its spectrum.

Two similar alcohol lamps being simultaneously employed on a certain preliminary occasion, one was looked at direct with the telescope tube as a cosmic phenomenon, while the other was placed in the standard manner on one side of the tube as a reference lamp inside a dark lantern. The spectra of the two lamps were then seen in the prism eyepiece spread horizontally, one over the other, and every band and every line of the one was standing in exact vertical coincidence over those in the other, saving a very little ragged light along the base line of separation of the two spectra, but which was of no real moment. There could then be no reason, on substituting the zodiacal light for the one lamp at the end of the tube, why the zodiacal light's spectrum, if bright enough, should not be seen honestly and fairly standing over the lower spectrum derived from the side reference lamp.

Our place of observation was unexceptionable. A window looking west out of the upper part of the Royal Observatory of

Palermo, and that observatory again standing on the summit of the Royal Palace, at 120 feet above the ground; that, too, a rising ground, looking far over the house-tops of the city, at a horizon formed of distant dark hills. A few lamps from a square far below did shine up at us, but a black cloth spread over the balcony cut off their rays; and every light was excluded from the apartment we were in, excepting only the reference-lamp of the spectroscope, and that was allowed merely to throw a small portion of its faint blue base of flame into a concealed part of the instrument. The slit of the spectroscope, too, was perfectly protected from stray side-light, for it was at the bottom, or inner end, of the blackened telescope tube of the apparatus—a tube originally intended to carry a large collecting object lens, but the use of which lens was found rather prejudicial than otherwise as to brightness on so broad a surface as that of the zodiacal light.

From that window, then, of the darkened room at the top of the palace, we looked out to the west, and there was the zodiacal light brilliant, so to speak, instantly recognised by all of us as being, as it should be, for the vernal equinox in Lat. 38° N., and vastly brighter than it ever could be seen in any very northern city as of 50° or 60° Lat. Upon that most visible zodiacal light, then, we directed the grand tube of the spectroscope, its slit being arranged narrow, or as most suitable to resolving the reference of blue flame bands into lines; or, as I may add from long Edinburgh experience, for seeing and identifying the one bright line formed by the peculiarly monochromatic light of ordinary green aurora. On looking into the eye-piece, there were all these bands of the reference spectrum with symptoms of linear resolution, together with the lithium and sodium lines thin, as representing the fine slit, ranged across the lower part of the field; but in the upper part of the field where the spectrum of the zodiacal light ought to have appeared, there was nothing. To make quite sure, the instrument, carrying the reference-lamp along with it, was moved slowly, first in azimuth and then in altitude, backwards and forwards across all the brightest parts of the zodiacal light; the reference-lamp, too, was dulled, and at last altogether excluded, but still nothing whatever was seen on looking with the prisms through the slit at the zodiacal light, which otherwise was not only abundantly bright to the naked eye, but also when viewed through the same prisms if used without any slit at all.

The only explanation, then, that is possible for the non-appearance of any spectral light, whether in lines, bands, or anything else, when the fine slit was used, is, that the light of the zodiacal manifestation is not, as has been sometimes asserted, monochromatic light of one definite refrangibility (in which case it could only give a line as bright as the full phenomenon, and as narrow as the slit), but it is of many various refrangibilities, and

spread thereby over so large a spectral range as to become weakened down to practical invisibility.

To ascertain over how great a spectral range the zodiacal light is spread, the slit was opened slowly until *something* was seen in the dark field, and then it was no line or lines, but a short portion of continuous spectrum that was indicated, becoming clearer, and rather larger too, when the slit was still further widened. Indeed we made the slit at last even extravagantly wide, or approximating in such breadth to the length of the said portion of continuous spectrum itself.

This portion, however, was never definite, never bounded by sharp upright parallel lines, as were the lines of lithium and sodium with that broad slit; but it was shaded off gradually into darkness at each end, and had its general maximum of light rather nearer the less refrangible end. Moreover, it was altogether so faint, as well as undefined and indefinable at the same time, that it could never be seen certainly in conjunction with the reference spectrum, the latter having to be excluded and then let in again, so that the comparison of the two might be made by memory assisting the eye.

These characteristic features of the zodiacal-light spectrum are represented in an accompanying plate in its spectrum pairs 1, 2, and 3, and are, I believe, substantially agreed to by Signor Cacciatore and Professor Tacchini, as well as myself.

Comparison of the new results with older ones already published.

To prevent any needless delay, as happened to a similar case last year, in examining, weighing, and publishing these results of observation, I refrain from mentioning the *names* of the various leading spectroscopists of the day, by whom totally opposite conclusions from those now detailed have been arrived at; it is sufficient for the scientific discussion of the case as it exists in nature, to mention what these distinguished astronomers have concluded, and to remind that their decisions have hitherto been generally accepted by the public, and are considered to have proved, that the spectrum of the zodiacal light *is the same as that of the aurora*.

And what *is* the spectrum of the aurora like, according to these authorities?

The chief part of them say that it is for the ordinary pale aurora simply monochromatic, or consisting, with a fine slit, of a single and sharp definite greenish line near W. L. 5579; yet some amongst those authorities there are who stand out for a rather brilliant continuous spectrum also. These latter persons, however, I do not scruple to say, are in error. They were generally those upon whom, in Southern European latitudes, the splendid aurora of February 4 came so astoundingly and unprecedentedly

that they rushed out—all inexperienced—with chemical spectroscopes, telescopic-star spectroscopes, sun-prominence spectroscopes, and anything at hand, however inappropriate, and hazarded a spectroscopic shot at the unusual phenomenon, under circumstances exciting, fascinating, agitating, to the last degree ; and never repeated since. I saw too, some time afterwards, also in a Southern university, how a grand continuous spectrum might have been incontinently obtained by the use of one of those instruments, even out of, or with the monochromatic green light of aurora, and from the red portion too on that unique occasion.

The party in question had a magnificent 4-prism chemical spectroscope, and desired very obligingly to show me the D lines in daylight from the blue sky, as seen through a window of the apparatus-room. The party laboured and laboured, but could get no lines at all, only an impure continuous spectrum. At last a friend of rare skill arrived, who discovered that the portion of light entering direct through the slit was swamped by the superabundance of the light entering slantingly *behind the slit* into the hole of vision in the side of the brass box covering the prisms. This was an effect which would not have occurred if the source of light had been small, but was thus destructively powerful when the source was of large area, as it was undoubtedly either with the said window, or still more with the aurora of Feb. 4, for that was on all sides of the heavens at the same moment.

What then, with auroral light pouring unexpectedly into the spectroscope by a circular hole 1·3 inches in diameter, the indiscreet use of lamps, and the insufficient avoidance of other lights on an *impromptu* occasion of promiscuous observing, there is no difficulty in explaining the false continuous spectra once ascribed by certain persons to the aurora, even when using a fine slit. Hence we may fall back in confidence on the accounts of the larger part of spectroscopists who say, and say truly, with the first of all auroral observers, M. Angstrom, that with a *fine slit* on an *ordinary aurora*, they see only one fine or thin greenish line.* So fine, sharp, and bright, indeed, is this line, or so undiluted by dispersion, that even on faint beams of aurora, not brighter than the zodiacal light, and with a fine slit, of course, I have myself used three times the dispersive power that that latter phenomenon would not bear even with a broad slit ; and still, not only have I then seen the auroral line distinctly, but compared its place neatly against that of the second line in the citron band of the blue base of alcohol flame in W. L. 5579.†

* I am confining myself here to an ordinary, or greenish aurora, and a fine slit ; what may be seen on a red and extraordinary aurora, and with a wide slit, I have already described in the *Edinburgh Astronomical Observations*, Vol. XIII.

† See Spectrum-pair 7 in the accompanying plate ; also the plates in the XIIIth Volume of *Edinburgh Astronomical Observations*. I have since then learnt, by correspondence with Dr. W. M. Watts, and by reading his useful little book, *Index of Spectra*, that each of the fine lines which I describe in the

If, then, the true state of the case stands thus, that the ordinary auroral fine-slit spectrum is one thin fixed line, how can those persons be justified who assert that it is the same spectrum as that of the zodiacal light, viz., that very one which we have shown, I hope, incontestably, to be a most diluted and dispersed portion of continuous spectrum, broad, indefinitely shaded off at either end, and far too faint to be appreciated by the human eye when employing a narrow slit and prisms of a decent amount of dispersive power? My answer must stand thus:—

1. Some of the observers were in high latitudes, and had not eliminated their frequent auroral glows from their only occasionally supposed and most faint zodiacal light. They ought evidently to try their observations over again in parts of the world where there is less aurora and more zodiacal light.

2. None of the observers, whose accounts I have been able to read, employed any reference spectrum to prove either the focus, breadth of slit, dispersion, place in spectrum, or any other necessary condition under which they were observing: and there are documents against some of them, connected with their alleged places of the principal aurora line on Feb. 4, showing that they cannot safely dispense with such instrumental check and guide, even in a far easier case of observation than that of the zodiacal light.

3. The spectroscopes employed were generally little portable things of such small dispersion but very tall slits, as to make what was really a broad tract of continuous spectrum to look like a narrow band, or an ill-defined line; and this appearance being seen in an instrument whose focus may be rather uncertain at night, and which has on previous occasions shown the auroral line instrumentally blurred, is hastily declared to be a line also.

4. Other observers, again, who, with a broad slit, have seen the continuous spectrum of the zodiacal light, and have marked a fine bright line, like that of the aurora when seen with a fine slit though in a different spectrum place, down the middle of it, have let their fancies run away with their judgment and have pictured that which is spectroscopically impossible, considering the large surface of the zodiacal light, no matter what the physical characteristics of that light may be, monochromatic, polychromatic, or anything else.

5. And, finally, those observers who have painted a very *brilliant* picture of the zodiacal light spectrum, whether continuous or linear, have failed in representing one most characteristic feature of the fact, viz. its faintness.

In short, though I am sorry to say it, none of the observers who have declared for the spectra of the zodiacal light and aurora

coloured bands of blue flame base, or acetylene, or carbon, breaks up, under higher dispersion, into a finer species of lines still; but *their* places do not seem to have been determined yet by any one; and his final wave-length numbers still therefore apply to my "lines," though to his bands; my "bands" being his groups.

being the same, have proved any of the necessary elements of their observing methods.

Phosphorescence Spectra, Astronomical.

Not only, however, have we to deal in the present day with the above too often published misleading equation between two grand physical apparitions, but there is another equation of the same order put forth by some of the same high authorities; viz., that on certain nights when "there is a general phosphorescence over the whole sky, the auroral line (W. L. 5579) may be detected in that light equally over it all."

Of course, if the aurora *itself* is spread on any occasion over the whole sky, its line must be spectroscopically recognisable everywhere; but phosphorescence is not aurora, and aurora is not phosphorescence; and that which some observers mysteriously call by this name "phosphorescence," and have never attached any peculiar spectrum to, is, in my humble apprehension, nothing but star-shine and night-light, similarly with sunshine and daylight. Starshine, of course, exists for every one who can see the principal stars; and the night-light of the background of the starry sky is just as certainly a fact, being made up, not only of the reflected light of those brighter stars on our own atmosphere, but of the direct light of myriads of smaller stars far too minute to be individually appreciated by the human eye.

And what sort of spectrum ought such star-shine and night-light to offer to the observer? Not a sharp, aurora-like line at all, but something, according to the manner and locality of its formation, very much like the spectrum of the last twilight of day. In fact, according to numerous observations on the Sicilian night sky, when free from any accusation of aurora, I found such night-shine to yield a short continuous spectrum; and that culminating, not at the aurora line place of W. L. 5579, but near W. L. 5350.

Now this place evidently corresponds within the limits of error of observation to that of the last residual portion of the continuous spectrum of stronger daylight or sunshine (with the eclipse corona line also, at W. L. 5322), modified slightly by the absorbing and reflecting powers of the atmosphere.* No Fraun-

* On the 6th of April, in Palermo, I made the following observations with the auroral spectroscope, but turned now upon the ordinary evening twilight, the slit being fine or proper for seeing Fraunhofer lines at midday. The alcohol reference-lamp was also employed to give further proof of the state of the slit and to indicate the spectral position of anything that might be seen in the twilight spectrum; which spectrum, in less than an hour, passed through all the following stages.

Stage 1. A continuous bright-coloured spectrum, showing the usual solar and telluric black Fraunhofer lines distinctly.

Stage 2. Continuous spectrum faintly coloured; Fraunhofer lines have now disappeared; but are replaced by darkish bands, probably formed of aggregation

hofer lines are seen in such star-shine spectrum for these reasons : 1st, the continuous spectrum is too faint to show any black lines upon it clearly ; and 2nd, the slit used at the time of observation was too broad to have shown Fraunhofer lines even in noonday shine. Indeed to see the spectrum of general night starshine, the slit of the spectroscope had to be opened wider still than in the case of the zodiacal light ; but gave then a closely similar spectrum ; and thence we are led to the conclusion that the spectrum of the zodiacal light is the same in kind as that of either star-shine or faint sunshine. Whence the further deduction is inevitable, that the older astronomical theory of the zodiacal light being the solar illumination of infinitely small distant particles such as meteors revolving about the Sun, whether in orbits of infra or ultra planetary ellipticity, is spectroscopically maintained ; while, that such solar zodiacal light has any physical connexion with the essentially terrestrial accompaniment of aurora, is just as eminently negatived by the spectroscope : for "no two spectra," as the lady most truly said in the Royal Observatory at Palermo, "can be more essentially different than those of the aurora and the zodiacal light. They are as different from one another as night from day."

Phosphorescence Spectra, Terrestrial.

Yet though our spectroscopic results so decidedly spoke for a solar origin and cosmic nature to the zodiacal light, one of the Italian mainland astronomers has published views very recently, dragging down the said phenomenon not only to the terrestrial region of the aurora, but to the still lower topographical level of meteorology and the bad weather experienced at his own Observatory in the beginning of March.

The weather with him had certainly been very bad during those days; but I had undergone some portion of it myself, having been passing along the Mediterranean from Gibraltar to Palermo at the time ; and though, whenever the evening sky was free from clouds, the zodiacal light was conspicuously bright, I

of said lines, combined with the increased haziness of dark lines on a darkish back-ground (a natural law well known to, and made much use of by, successful painters of night scenes) : the preponderance of light belonging to the continuous spectrum, is very manifestly in the green region.

Stage 3. The green portion of the continuous spectrum is alone visible.

Stage 4. Green portion is shorter ; shaded off indefinitely at either end, and culminating in what brightness it has near or about W. L. 5300.

Stage 5. Too faint now to be seen simultaneously with reference-spectrum : is seen again on widening the slit.

Stage 6. Too faint again to be seen simultaneously with reference-spectrum, even with a broad slit ; but on excluding reference-spectrum the twilight spectrum may again be seen, and is found to resemble in shape, position, and all visible characters the zodiacal light spectrum as already described. The general starlight spectrum, or the spectrum of the night-shine amongst the stars above the reach of twilight or zodiacal light, is also of the same order, but fainter, and requiring a broader slit to be appreciated at all.

could not see, and the said theorist has made no attempt to prove, that it was in any way, or degree, brighter than it should have been for that latitude in that season of the year; that season, *i. e.* the beginning of March, being, let it be remembered, the long since astronomically determined date of annual maximum evening brightness of the zodiacal light for all places in the northern hemisphere.

The bad weather was accompanied no doubt over my track by a grand development of *phosphorescent* light; but that phosphorescence was in the sea, not in the sky, which, when clouded, was dark enough; and I should not think of occupying the attention of the Royal Astronomical Society with any account of it, had not that strange quality, "phosphorescence"—by name, at least,—been imported into zodiacal light discussions by so many eminent and great men; and had I not succeeded at last in doing what I believe they did not, viz., comparing such phosphorescent spectrum with that of the zodiacal light, as well directly, as through the medium of the reference-lamp.

Now, the phosphorescence here spoken of was peculiar. It was not composed of the little bright stellar sparkles, nor of the globes and discs of fainter light which are frequently seen in the wake of any vessel in many seas; but it was a broad and long trail of nearly uniform milky light, brightest close under the stern of the ship; so bright indeed there, that one could hardly but fancy that there were powerful lamps pouring forth their light from lower stern windows upon the foam-trail of a paddle-steamer. But there were no such stern windows in the "Kedar," and she was not a paddle, but a screw, steamer, keeping the blades thereof always well submerged and preserving by day a dark trail astern free from froth, foam, or bubbles, even under a noon-day sun; yet as soon as night set in, there was this phosphorescent tail, some 8 to 10 feet broad, and of indefinite length, stretching far away in luminous magnificence behind her.

At that time I had only an insufficient hand-spectroscope at command; but on finishing the zodiacal-light observations at Palermo, the same instrument, reference-lamp and all, was transferred on April 8th, to the SS. "Marathon" (belonging to Messrs. Burns and MacIver, of Liverpool), where I was to spend the next five weeks, and where the captain (John Leitch, Esq.), kindly consented to let the instrument be mounted in permanence on deck, so as to be ready for observation at any moment; a condescension to landsmen's science, in a neat and trimly kept ship, as unusual as it was in this instance both gratifying and effectual.

Night after night, however, followed, and there was for a long time very little luminosity of any kind behind this new ship; the Moon, too, was often over bright; and so it happened, that not until the 6th, 9th, and 11th days of May, in seas on either side of Gibraltar and near Lisbon, was there any opportunity of making the long-desired observation of the spectrum of light from a

similar broad sheet of milky phosphorescence to that so often seen in the "Kedar," Capt. Pritchard. The observation, however, was most satisfactorily taken on the above-mentioned days, up to the limits of power of the instrument, and gave for result,—a short continuous spectrum in the green, culminating at about W. L. 5050; or totally different from the places of both the aurora and the corona lines, much further on in refrangibility than the zodiacal-light spectrum, and having its greatest intensity towards the further, and not the nearer, end of its own light; as will be seen in the diagrams of spectrum pairs 5 and 6 in the accompanying plate of spectra symbolically represented.

Addition to a paper on Errors in Vlacq's ten-figure Logarithms, published in the last number of the Monthly Notice. By J. W. L. Glaisher, B.A., F.R.A.S., Fellow of Trinity College, Cambridge.

The main object of the present addition to my former communication is to correct an inaccuracy therein, and to add several errata with which I have since become acquainted. The inaccuracy (which is of no importance, as far as the object of the paper is concerned), is contained in the following sentence which occurs on p. 259:—"Lefort's list contains 452 errata, 301 of which are marked with an asterisk, to imply that they were given by Vega;" the words of Lefort's note are, "Toutes les fautes, relative aux nombres qui sont marqués d'un astérisque, se retrouvent dans le *Thesaurus logarithmorum completus* de Vega;" and this means, as is evident on examination of any one of the marked numbers, that the errors in question are reproduced in Vega, not that they are pointed out by him; the words, "to imply that they were given by Vega," should therefore be replaced by, "to imply that they occur also in Vega." It does not follow that the number of errors found by Vega in 452 — 301 = 151, as Vlacq's tables proceed steadily from unity to 100,000, with differences throughout, while Vega gives the logarithms of the first thousand numbers (without differences), and then the logarithms of numbers from 10,000 to 100,000, with differences; so that no errors in Vlacq's differences, &c., below 10,000, are noted; this, however, makes but slight practical difference, as this portion of Vlacq is never used.

In my paper I stated that "I had made no examination of Vega's errata-list," feeling, no doubt, that this was completely done by Lefort; but by the aid of it I have since been enabled to add to my list on p. 258, the three following errata in Vlacq's, which are not given in the *Arithmetica*, or by Lefort:—

Number.	Error.	Correction.
26517	43889	43886
30134 Diff.	1,44116	1,44119
64818	56295	56265

The first two of these are mentioned in the preface to Vega (where also reference is made to 11293, and 18723, given on p. 258), and are printed correctly in the table; the last appears in another list at the end of the introduction, and is not printed correctly in the table.

The copy of the folio edition (1794) of Vlacq, which I have always made use of, is that in the Cambridge University library; there is a preface containing a list of numbers in the logarithms of which errors were found in Vlacq; and at the end of the introduction are two pages of additional errata (in Latin and German), which the possessor of the work is requested to correct in the tables before use. The preface thus contains errors found in Vlacq before the printing of the tables, while the second list gives the result of an examination of the printed tables. All the errata in the preface are therefore errata in Vlacq, while the errata in the second list are errata in Vega, and may or may not be also errata in Vlacq: on this account, those that are also errata in Vlacq have an asterisk prefixed. The note explaining the meaning of this asterisk is not at all clear, unless the preceding explanations are premised; but it will be found that errata without the asterisk occur only in Vega, while errata with it have reference also to Vlacq, and are supplementary to the list in the preface. One or two numbers appear in both lists, and the number, 38889, should have an asterisk.

I was not a little surprised on looking at the copy of Vega (folio 1794), in the library of the Royal Observatory at Greenwich, to find that the *Corrigenda ante usum* at the end of the introduction, consisted of only one page; that most of the errata marked with an asterisk in the Cambridge copy were omitted; and that none were marked with an asterisk. The edition is also much better printed on whiter and thicker paper. The inference is, that after the publication of the table, the additional errors in Vlacq were found: the original *Corrigenda* page was therefore cancelled, and replaced by the two previously described. This affords an explanation of the fact that Lefort does not notice Vega's *Corrigenda* at all.

It ought to be mentioned that Vega does not do himself justice in his preface, as he has corrected many more of Vlacq's errors than he has called attention to; for example, errors are corrected in 38578, 48359, 40909, 42344, 41407, &c., those in the last two being important.

It is not very easy to determine in what manner Vega examined Vlacq's table, but it is very likely that he verified all the differences (and therefore all the numbers) by subtraction. Any error in the differences, unless very small, would also be rendered apparent in the course of re-arranging them as they appear in Vega. I have not yet made a detailed examination of Vega, nor have I compared side by side the two copies.

There is another error in Lefort's errata list which must be added to those on p. 258.

	Number.	Error.	Correction.
viz.	11003	29	30
which should be	11003	39	30

The origin of this inaccuracy is clear: Vlacq has 39, the 3 being a misprint for 2; Vega corrected this, and printed 29; the remaining unit error of course is an error of calculation.

It would be very useful to examine all the errors in Vlacq which could affect a table of seven-figure logarithms, and give these in a list by themselves: such a list would occupy little space, and would be very valuable, as by means of it, anyone could easily render any seven-figure table free from the hereditary errors that have descended from Vlacq. In a paper entitled, "Notice respecting some errors common to many tables of logarithms" (*Memoirs of the Royal Astronomical Society*, vol. III. 1829), Babbage has noticed six errors that occur in Vlacq, and have been reproduced in nearly all subsequent tables. Out of 22 tables which he has included, the errors occur in all that are intermediate to Vlacq and Vega, and in several that are more recent, the only ones free from them being the Vegas, the later editions of Callet and Hutton, and Babbage. Even tables published since Babbage's contain some of these errors: thus five out of the six appear in Shortrede, and one in Sang's tables (1871). Another of Vlacq's errors also appears both in Babbage and Sang (see *Athenæum*, June 8 and 15, 1872), so that even by editors of logarithmic tables all the Vlacq's errors that have been published are not known.

This suggested list of errata in seven-figure logarithms, which, by the aid of Lefort, can now be rendered probably really complete, I intend to form at once.

The following errata in former paper may be noted here, viz. p. 258, line 4, "Diff" should be inserted after 3192; p. 259 (note), Hatton should be Hutton, and p. 262; "Leipsiæ" should be "Lipsiæ."

Eclipse Photography. By A. Brothers, Esq.

It may, perhaps, be thought remarkable, that the photographs taken in India during the Eclipse in December last, do not show the outer corona, and its absence from the negative would almost lead to the supposition that the outer rays had no existence during this eclipse. But as we have the evidence of Captain Tupmann, that he traced those outer rays to about 40′ from the Moon's limb, the inquiry naturally arises, why do not the photographs show this part of the phenomenon?

I am strongly of opinion that any such effect of light visible to the naked eye, if of sufficient duration, can be photographed; and, as the outer corona is persistent during the whole time of

totality, and clearly visible in the No. 5 Syracuse negative, it is, I think, desirable to consider the reasons why this single photograph shows an effect which is not seen in any of the photographs taken at Bekul, or at Dodabetta.

The No. 5 negative was exposed 8 seconds, and was developed in the ordinary way with iron, but not strengthened, care being taken that the development should not be pushed too far so as to obscure detail. As the denser part alone was visible in the dark room, it was only on the negative being viewed in the full daylight that the faint part of the corona could be seen, and then chiefly by reflected light.

That the effect on the plate is really a picture of the corona is proved in various ways. The light shades off gradually from the bright parts of the corona, and there is no photographic effect where the rifts occur. The faint corona was visible to the unaided eye, and was identical with the appearance photographed. Now as the negative is altogether untouched, why is it that it shows an effect which none of the Indian negatives exhibited? The reason appears to be that the difference is probably due to manipulation. The No. 5 negative shows what was actually visible, but it happened that the exposure and developments were just right for bringing out such detail.

A careful examination of Mr. Davis's negatives will show the cause of the difference between them and the Syracuse picture.

The Bekul negative No. 1 was exposed 15 seconds, and shows a corona extending about 15′ from the Moon's limb.* No. 2 was exposed 10 seconds, but shows a corona of only about 8′ extent. No. 4, however, was also exposed 10 seconds, but shows the coronal rays extending in some directions about 20′, with indications that they reached a still greater distance from the Moon. As the conditions were, I believe, the same during the whole time of the eclipse, there would be no variation in the light to account for these differences, and we must fall back on the manipulation and time of exposure for our explanation.

As I have not seen Colonel Tennant's photographs, I cannot refer to them in detail, but it has been stated that they present very much the same appearance as those taken by Mr. Davis, and my remarks therefore refer to the Bekul negatives only.

Those of the Fellows of this Society who are familiar with the practice of photography, will at once understand what is meant by manipulation; but to make my intention quite clear, I have prepared three photographs to illustrate the matter. They are copies of my drawing from the Syracuse negative. The exposure for each picture was the same; 5 seconds, and in the same light. The print marked A presents only the bright part of the corona; B shows the proper effect of the drawing; and C shows the effect

* I am speaking with reference to the transparent copies kindly sent me by Lord Lindsay. The original negatives will probably show different measures, and other transparent copies will differ; those I have, are very perfect and beautiful; but the argument is the same in any case.

of pushing the development too far.* The examination of these photographs leaves no doubt that such a faint object as the outer corona would only be depicted on the plate when the development and time of exposure had been exactly suited for it — under-development in the one case would prevent its appearance on the plate; and in the case of over-development, it would be quite possible to obliterate all trace of it.

Seeing then that of the six or seven good pictures taken during the late eclipse, not one shows the full extent of the outer corona, it must be concluded that the operators have not succeeded in so accommodating the exposures and development as to obtain the result which the Syracuse picture fortunately, but as I am inclined to believe accidentally, shows.

The failure, however, in obtaining the full picture of the corona, may possibly be explained in another way. It was early morning when the Bekul photographs were taken; and at Dodabetta the eclipse was observed through a mist. This mist was not sufficient to obscure the details of the inner corona, but photographically the interference may have been fatal for the fainter rays. Those who have attempted to photograph the Moon know very well the effect of the slightest mist in the atmosphere; and it must be borne in mind that the outer coronal rays are fainter, very much fainter, than the light of the full Moon.

At the last Meeting of this Society, Colonel Tennant is reported to have said, that "of course" the photographs do not show all the eye could see. From what I have said, and from the fact that the Syracuse picture does show what the eye saw, I cannot entertain Colonel Tennant's opinion. What the eye can see can be depicted photographically (the atmospheric conditions being favourable), but I am not at all sure that we have hitherto adopted the proper means to secure the best pictures of an eclipse. It is probable that the Syracuse photograph is one of the fortunate successes which occur in the experience of every photographer. Following the same means, it is not certain that the same thing can be done again; but there is strong reason for the opinion, that by using the old, but now almost forgotten *positive* process, the kind of picture required could be obtained with certainty. A positive on glass gives the picture by reflected light; but if the picture be viewed by transmitted light, we have the effect of a negative. We by this means secure two objects—the brighter corona, which requires so much less time to impress its details, would be seen by transmitted light, and the full picture by reflected light.†

The opinion has been expressed that better work could have

* These photographs were exhibited at the meeting, and will be again shown at the meeting of November 8.

† With the box of chemicals, &c. which I prepared for Colonel Tennant, I sent everything requisite for working the positive process, thinking it probable that some of the pictures might be taken in the way I suggested; but as only negatives have been spoken of, I conclude the suggestion was not acted on.

been done with a reflecting telescope during the last eclipse, than with the rapid rectilinear lens used by Colonel Tennant and Mr. Davis. When we bear in mind what has actually been done by the various classes of instruments, it is difficult to find a reason for the opinion referred to. It cannot be said that the refracting telescope has altogether failed, as I have before me as I write, a very beautiful photograph of the corona, printed direct from the original negative taken at Shelbyville by Mr. Whipple, in August 1869. We have also the picture taken in Spain in 1870. The Shelbyville photograph is $\frac{7}{8}$ of an inch in diameter; it was exposed 40 seconds, and shows a corona extending about 6′. The Cadiz picture is less perfect, but it was exposed $1\frac{1}{2}$ minutes. In these two photographs we have the best work hitherto done with the ordinary telescope. (I do not refer to the 1860 photographs, because the prominences were the chief objects to be depicted, and the corona shown is very limited in extent.)

The silvered-glass reflector was used first in India in 1868, for the purpose chiefly of photographing the red prominences, and the negatives were obtained with exposures ranging from 1 to 10 seconds. Judging from the plates engraved in the *Memoirs* of this Society, the longest exposure was not sufficient to show the corona. A similar reflector was used by Lord Lindsay in Spain, but from some reason not hitherto explained (possibly owing to atmospheric causes) the corona is not seen in a satisfactory manner. It is probable, however, that owing to the focus of the telescope being short, as compared with the aperture, there is more light around the dark Moon than in any other eclipse photograph taken with a reflector.

The larger image obtainable with the instrument of long focus is its only advantage. As, however, a longer exposure is requisite, we lose in one way all the advantage gained in the other. Instruments of long focus are objectionable, on account of the large mounting required, and for other reasons.

On the whole, it appears to me that the evidence is in favour of an achromatic object-glass. I do not maintain that the rapid rectilinear lens is the best that can be had, but it is possible that a lens of similar construction of large aperture, and giving a picture of the Sun of about half an inch in diameter, would work more satisfactorily than a reflector of equal proportions. Such instruments, as I have indicated, could be tried side by side on the Moon. The best effect with the shortest possible exposure are the points to be tested. The most perfect driving clock in the world will not avail against atmospheric disturbance.

When the *extent* of the corona is referred to, it would be advisable, in all cases, if the observer would state whether a naked eye or telescopic view is referred to, as it appears to be pretty certain that the telescope fails to show more than the brighter parts.

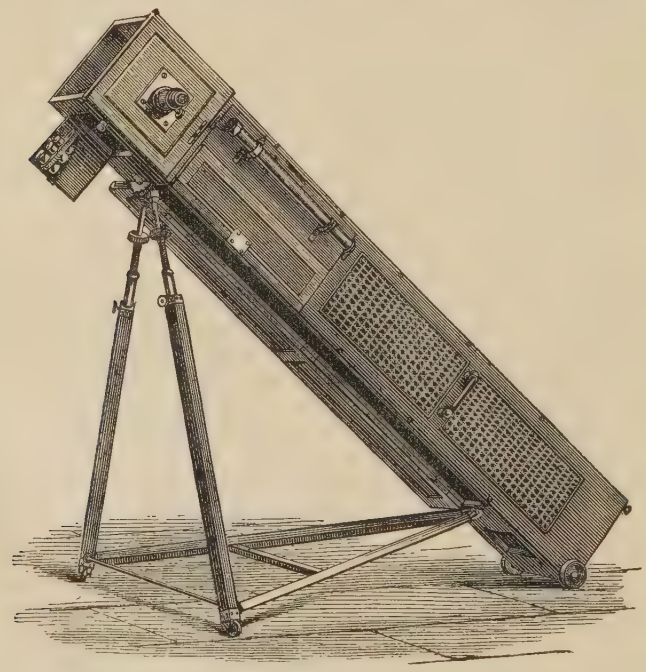
It should be distinctly understood that in what I have said I disclaim all intention of undervaluing the work of others, as I

think what has been done is so near perfection that it is difficult to conceive anything superior to be done, when the particular character of the corona is taken into consideration.

June 12, 1872.

On an Altazimuth Mounting for Telescopes, especially adapted for the use of Observers who have no permanent Observatory. By John Brett.

The engraving intended to illustrate this paper represents a reflecting telescope of the Newtonian form, of $9\frac{1}{4}$-inches aperture and 6 feet 2 inches focus, the mounting of which exhibits certain peculiarities intended to improve the performance of such instruments, as well as certain contrivances calculated to render them



more portable than usual, and more readily available for general use. The principle upon which this mounting depends is applicable to any sort of telescope; but inasmuch as steadiness and smoothness of movement are of comparatively easy attainment in refractors, I propose only to lay before the Society my adaptation of it to reflectors.

The telescope is supported at both ends; the pivot on which

it revolves is placed at the lower end immediately under the speculum; and the power by which it is moved and governed is applied as far as possible therefrom, that is to say, at the eye-piece end of the tube. This disposition is intended to prevent any tendency to oscillation, and to check any vibration that may be set up by the wind or by any accidental cause.

The whole apparatus, telescope and stand, may be briefly described as a tripod with a moveable apex; and the essential peculiarity (and, as I suppose, novelty) of the mounting consists in the means by which the apex is made to move in any required direction.

One leg of the tripod consists of the tube or body of the telescope, the speculum end of which rests on the ground, whilst the eye-piece end, being situated at the apex, is supported by the other two legs.

The telescope having a given length which is invariable, it is obvious that it may be made to stand at any angle to the horizon by the extension or shortening of the other two legs. It is further evident that if these two contractile legs be shortened or lengthened at the same rate, the telescope or third leg will be moved merely in altitude; that if one of them only be extended or reduced the telescope will describe an arc of a circle; whereas, if one leg be lengthened and the other shortened at the same rate, a lateral movement will be communicated to the apex, that is to say, the telescope will move in azimuth only.

It will be seen that the figure of the whole apparatus is that of the simplest of all solids, and since its movements depend entirely on thrust—torsion being wholly excluded, and friction reduced to a very small amount—it is hardly possible to manufacture it so weakly as to produce unsteadiness.

The mounting provides for quick and slow motion both in altitude and in azimuth, besides which it affords a slow motion in a circular direction, which latter may be made equatorial by placing the speculum end or pivot of the telescope on a bench or other support so many degrees higher than the casters of the other two legs as shall equal the latitude of the place of observation. The quick motion in altitude is given by an extension of the two supporting legs, the upper halves of which (consisting of two similar brass tubes about an inch in diameter and about 28 inches long in the particular instrument referred to) are made to draw out of the lower halves, which consist of mahogany cylinders or boxes carrying clamping-screws to fix the brass tubes at the required height. The quick motion in azimuth is given by simply moving the whole tripod on its casters. The slow motion in every direction is communicated by two double-threaded steel screws (18 inches long), which work in the brass tubes and are moved by the observer. The heads of these screws terminate in ball-and-socket joints permanently fixed to the tube of the telescope. Both these screws may be turned at the same time by the observer using both his hands, and the result will be that when both

are moved in one direction the telescope rises in altitude, or in the other direction it descends ; but if they are turned in contrary directions the instrument moves in azimuth. The rotation of one screw alone produces a circular motion which, affording as it does a certain approximation to the apparent motions of the stars, allows the observer to keep the object in the field with greater facility than the ordinary altazimuth mounting does, whilst the equality of force required to rotate either of the screws and the smoothness of their action allow the object to be followed without any care or attention, and far more steadily than by the usual Hook's joint.

In this instrument the legs are connected together by slight bars of wood which can be readily folded along with the legs; and when not in use the whole stand, together with the finder, nose-piece, eye-pieces, and all other appendages, retire into the seclusion of the telescope-tube, the door of which shuts them off from the meddling of the curious by closing with a spring-lock. The tube in this instance has been made rectangular, as the cylindrical form was supposed to lend itself favourably to vibrations. The upper side is covered in with panelling to prevent the deposition of dew, but the other sides are filled in with cane-work to prevent air-currents. The door at the upper end is made to open so as to bring the nose-piece exactly opposite to the plane mirror. The door at the lower end allows the cover to be taken off the speculum. The little door underneath carries the eye-pieces, and keeps them close to the observer's hand and away from his pockets. The hollow legs are furnished with spiral springs, which form a counterpoise of increasing power as the weight of the telescope is thrown forward upon them. Here ends the description of the instrument.

It being generally recognised that unsteadiness is alone sufficient to render the best of telescopes utterly useless; that reflectors are peculiarly liable to suffer from this defect ; that in order to avoid it great weight and massiveness have almost always been resorted to in their mounting, and that this cumbrousness tends very much to restrict the use of large reflectors to permanent observatories, thereby crippling to some extent the observing power of this Society : supposing that these things are generally admitted, I have thought the characteristics of portability and steadiness to which my principle lends itself with a certain aptness give to this apparatus sufficient claims to attention to warrant my producing it for the inspection of the Fellows ; and, if they do not think favourably of the design, they will, I feel sure, appreciate the way in which Mr. Browning has carried it out.

6 *Pump Court, Temple,*
May, 1872.

On certain Phenomena surrounding the Sun's Limb as seen in the Telescope. By John Brett, Esq. (Abstract.)

The appearance to which attention is called may be briefly described as follows:—

When the Sun's disk is examined with a telescope it is seen to be projected, not against an even background of sky-colour of uniform darkness, but upon a patch of weakly luminous halo diluted gradually outwards to an extent of about half a diameter of the Sun beyond his limb, where it dies away and becomes imperceptible. The colour of this halo is of a delicate steel grey with a slight hint of green in it. Its inner region next the limb possesses a certain brightness for about 2′ or rather less, exhibiting some traces of a defined limit at that distance, beyond which the diminution of its light proceeds rapidly. The aperture of the instrument does not seem to affect its visibility much, but the dark glass must not be darker than is necessary for safety. The inner or brighter belt has been seen best with a magnifying power of from 50 to 100 on Mr. Brett's 9¼-inch telescope, but, speaking generally of the whole phenomenon, the lower the magnifying power and the larger the field the more perfectly it can be seen. In a small finder, with field of 4° and magnifying power of 5, its appearance is remarkably vivid and strikingly like Mr. Brothers' photograph of the Corona of 1870, and on some occasions appearances exactly like the so-called rifts have been seen.*

By fixing a small strip of paper across the field-lens of a Kellner eye-piece so as to make a bar in the focus across the image, and afford a standard of contrast, the graduation of the light from the limb outwards was made to appear more distinctly, and since that date Mr. Brett has never examined the Sun's limb in good clear sky without detecting a similar appearance.

Mr. Brett has tried all the means he can think of to get rid of it, but in vain. He has shown it to several persons with good eyes (in various telescopes, with all sorts of eye-pieces and different dark glasses, first-reflection prisms, and films of silver), and most of them describe the phenomenon as perfectly obvious.

Mr. Brett invites attention to his paper in the *Notices* of this Society for March, 1871, in which is recorded some account of the Corona as seen during the progress of the eclipse of December, 1870, especially during its earlier stages. On that occasion the Moon's limb outside the Sun was seen by Mr. Brett and Mr. Burton projected dark on a brighter background of sky many minutes before the Sun's disk was covered.

6 *Pump Court, Temple,*
June, 1872.

* In a letter to Dr. Huggins, Mr. Brett mentions that the rifts thus seen are not permanent.

[The phenomena described by Mr. Brett, though not altogether new, afford an interesting subject of inquiry. The comparison of such phenomena as presented when different instruments are employed, when the Sun's image is in different parts of the field, and so on, may possibly throw light on questions relating to the performance of telescopes. It may be, on the contrary, that the phenomena are caused by the presence of matter in the upper regions of the atmosphere; that in fact it is a true atmospheric corona, and related to finely-divided matter very high in the atmosphere, in the same way that the common atmospheric corona is related to the water vesicles of *cumulus* or *cumulo-stratus* clouds. It seems by no means unlikely, indeed, that the phenomenon described by Mr. Brett is the "corona" caused by the particles which form the *cirrus* or *cirro-stratus* cloud. It may be interesting to compare Kaemtz's account of the corona of the cumulus or cumulo-stratus cloud. "If the corona is complete," he says, "several concentric circles are observed. Near the Sun they are of a deep blue, the second circle is white, and the third red, which terminates the first series; in the second we see, still going in the direction from the centre to the circumference, purple, blue, green, pale yellow, and red; the series is rarely thus complete. More frequently we observe near the Sun blue mingled with red, then a white circle clearly limited within, but confounded without with the others. If a second red circle exists outside this, then green is observed in the interval by which they are separated. The distance of this circle from the centre of the Sun varies according to the state of the clouds and atmosphere; I have found it from 1° to 4°." If we remember that such phenomena may be produced by reflected as well as by transmitted light, we see reason for believing that cirrus clouds, according to the usual belief as to their constitution, would produce a real though small corona as well as the haloes which it is their more special part to produce. In thus speaking of cirrus clouds, we have in view as well the dispersed matter which is almost constantly present in the upper air, though not recognisable in distinct cloud forms, as cirrus clouds more properly so termed.

It may be, however, that the phenomena are altogether optical; or, again, they may be merely subjective.

It is hardly necessary to show that the phenomena are not astronomical,—in other words, that the astronomical solar corona has not been seen. Dr. De La Rue has shown how hopeless it must be to attempt to see the prominences (whose light is certainly much brighter than that of the corona, even a few minutes only from the limb):* see vol. xxix. of the *Notices*, p. 81. The Astronomer Royal has tried the following experiment:—A smooth white surface, with a circular aperture, was so placed that

* In 1860, Goldschmidt could perceive a prominence which he had been watching, $3^m\ 40^s$ after the reappearance of the Sun, and believed he could have seen it longer had he not been compelled to withdraw from the observation.

the aperture exactly corresponded to the place where the Sun's image would fall, and the light thus passing through the aperture was quenched in a black bag. When this was done not a trace of the prominences or chromatosphere could be recognised round the aperture. If under these exceptionally favourable circumstances even the prominences could not be seen, it is certain that the corona cannot be seen when the Sun is not eclipsed (unless with some spectroscopic appliances not yet tried). It is sufficient to remark, however, as Lord Lindsay pointed out at the last Meeting, that since the corona cannot be seen through a darkened glass (such as is used in observing the Sun) during total eclipse, though then projected on a dark sky, it cannot possibly be seen through such a glass when the sky is illuminated by the full blaze of sunlight.

While on this subject we may mention a method by which possibly the whole circle of the prominences and sierra might be seen, either with red, yellow, green-blue, or indigo light, when the Sun is not eclipsed. It consists simply in repeating the Astronomer Royal's experiment, but viewing the ring or card through a train of prisms (after the manner proposed by Prof. Young early in 1871 for observing the corona, and applied by Mr. Lockyer during the last eclipse.) Theoretically it appears certain that the complete ring of prominences and sierra should thus be rendered visible at once. Or Sir George Airy's experiment could be repeated with the aid of the spectroscopic arrangement successfully employed by Respighi at Poodoocottah last December. Either method might probably be applied successfully to obtain photographs of the prominences and sierra, the indigo image being employed. It seems barely possible that a faint image of the corona might be detected by this method at the part of the aperture corresponding to Kirchhoff's line 1474. If the method failed under ordinary circumstances, it might be successful at a station very high above the sea-level. The present writer has been confirmed by the high opinion of the Astronomer Royal (to whom this extension of the black-bag method had also occurred), in his belief that valuable results might be obtained from the application of this method.

As regards the visibility of the lunar limb outside the Sun many minutes before or after totality, or in partial eclipses, it is only necessary to mention that theoretically this is to be expected. In an article in *Fraser's Magazine* for December, 1870, the possibility of thus detecting the Moon's limb outside the Sun's during the partial eclipse visible in England during that month was pointed out (before the event) by the present writer, who also can confirm Capt. Noble's statement that the lunar limb can actually be seen on such occasions. It is to be remembered that so soon as a solar eclipse commences the illumination of the atmosphere begins to be reduced, and that at an advanced stage of the partial phase this reduction becomes such that, under favourable atmospheric conditions, the Moon's limb

must necessarily become visible by its relief against the light of the corona (some time before the corona itself can be seen.)—R. A. P.]

Improvements in Tripod Stands. By R. T. Lecky, Esq.

My object in submitting to the Society so very simple a matter as a tripod-stand is, firstly, to place on record the invention of the well-known double, or, as it is commonly called, the French Tripod, so universally used under various forms for photographic cameras, portable telescope-stands, &c.; and, secondly, to show some further changes in its construction which may be usefully employed in various astronomical and geodetic appliances.

The invention of the double-stand is that of a gentleman to whom the scientific world is indebted for having laid the foundation of the vast improvements of later years in the construction of the object-glasses of microscopes, the late Joseph Jackson Lister, Esq., F.R.S., of Upton, Essex, and the stand which I have the pleasure of showing here to-night (kindly lent me by his son, Mr. Arthur Lister) is the first of its sort made in this country about forty years since by the late Mr. Bate, of the Poultry.

Mr. Lister was fond of sketching, and although well able to use his pencil unaided by any optical appliance, used the camera lucida for greater accuracy and speed, and as the ordinary walking-stick-stand was both cumbrous and inconveniently low, he contrived this to carry a small drawing-board, and to fold up and be used as a staff in his walking excursions. The upper portion of each leg is divided, and the short part is hinged to the side of the leg, and the two parts are then connected with a jointed link, which on shutting-up the stand fits into a recess in the side of each part, and the legs are then connected together with ferules, and a movable ring in the usual manner so as to form a light and useful staff. When the legs are opened out, the drawing-board is pinned on to the top and makes a firm table. Mr. Bate was so pleased with the invention that he requested Mr. Lister to allow it to be patented, but this Mr. Lister declined to do, and gave it to the public.

A few years since, on Mr. Lister showing me this stand, it occurred to me that if an ordinary level or theodolite tripod could be adapted to the same use by having a drawing-board braced on to its top; it might be made useful for a greater variety of purposes, and be readily made of materials common to every collection of instruments. I therefore tried a few experiments with the stand I have the pleasure to show you, and the result has been that by placing the board on a loose centre on the top of the tripod, and then applying two cross braces between the

side of the board and the two front legs, and two other braces as an isosceles triangle to the third, or back leg, and then connecting the legs together, the connexion between the front legs being rigid, I made a very firm stand or table, and with the unexpected result, that whereas, to use engineering terms, the first six braces, viz. the four between the sides of the board and the legs, and the two connecting the two front legs to the third or back leg, act as ties, the seventh or rigid connexion between the two front legs becomes a strut, and, as such, its action is to tighten all the other ties or braces, thus giving the table the requisite degree of stability.

The braces I have in this case made of spring steel for lightness, but they may be of chain or wire, and if with the strut were made adjustable, by screws or otherwise, the table could be adjusted as to level, or to any required degree of obliquity.

As to the different uses to which a stand of this kind is applicable, I may mention the old-fashioned but useful surveying instrument, the Plane Table, which, with this mode of bracing, may be made of much larger size than is usual, and be constructed with greater accuracy. It would also form a firm and convenient stand for various uses in the observatory as well as in the field; light, portable, and easily taken asunder and stowed in a small space, at the same time not interfering with the other uses of the tripod. Doubtless other uses will suggest themselves in practice.

Memoir on the Theories of the four Superior Planets, Jupiter, Saturn, Uranus, and Neptune. By M. Le Verrier.

An abstract of a memoir by M. Le Verrier on the theories of the four exterior planets is inserted in No. xxi., for May 20, 1872, of the *Comptes Rendus*. The subject is of great importance, and one deeply interesting to many Fellows of this Society. The following is a translation of the principal portion of the abstract.

"On several occasions I have had the honour of laying before the Academy a series of investigations relating to the system of the four planets nearest to the Sun; viz., *Mercury*, *Venus*, the *Earth*, and *Mars*. Although at an earlier period, I had already been occupied in the study of the larger planets, I have felt the necessity, before continuing the investigation, of establishing on a solid foundation the theory of the *Earth's* motion, which serves as the basis of all the others. This study led me to the consideration of the three planets situated nearest the *Earth*, and which constitute the inferior portion of the planetary system.

"These researches have shown that the motions of the *Earth* and *Venus* are represented by the theory with all the accuracy which the observations will admit of. *Mercury* and *Mars*, however, showed some irregularities.

"The difficulties which the last two planets presented to us have in a great measure disappeared after a careful examination of the subject. In order to establish an agreement between the theoretical and observed results, we have come to the conclusion that it would suffice if we were to increase the secular motions of the perihelia of *Mercury* and *Mars*, which would imply the existence in the neighbourhood, both of *Mercury* and *Mars*, of sensible quantities of matter of which we hitherto had no knowledge.

"With regard to *Mars*, these deductions have been confirmed. It has been proved that the matter, of which no account had hitherto been taken, ought to have been added to the *Earth* itself; the estimate of the mass of our planet being $\frac{1}{8}$th too small.

"With regard to *Mercury*, this confirmation is not yet complete. Several astronomers have placed on record the passage over the *Sun's* disk of certain small bodies, which could be nothing else but very small planets, but it has not been possible to determine the orbit of any one of them. Whether we have to deal with the action of a certain number of small masses, or with that of matter distributed in the neighbourhood of the *Sun*, the theory of *Mercury* has been determined with considerable care, and the transits of the planet over the *Sun's* disk furnish us with observations too precise to admit of any doubt of the accuracy of our results; especially as they have been obtained in the same manner as for *Mars*, and for this latter planet the confirmation which the theory has received leaves nothing more to be desired.

"Similar researches into the system of the four large planets farthest from the *Sun* would be interesting, as they would furnish us with data concerning any matter, still unknown to us, that may be situated in those regions. It is not impossible that, by these means, we might succeed in proving the existence of some planet beyond *Neptune*, and in limiting the space in which a search for such an object may be confined. At any rate we should have collected the materials necessary for the more speedy accomplishment of future discoveries.

"The first part of the work to be undertaken consists in the determination of the disturbances which each of the four planets experiences from the action of the others. It is this first part that I present to the Academy to-day.

"The respective theories of *Jupiter* and *Saturn* are not free from difficulty on account of the magnitude of the masses of these planets, and because the ratio existing between the mean motions of *Jupiter* and *Saturn* is very nearly represented by 5 to 2. Under these circumstances, some of the terms rise to a very considerable value, which requires the approximations to be carried farther. Besides which, the series of multiples of the angles, by following which the perturbing functions and perturbations are developed, have to be carried very far to avoid neglecting any term of the longitude exceeding one hundredth of a second, as we resolved to do. Again, the terms depending on the second order

in relation to the perturbing masses, become very sensible, and their determination is a laborious operation.

"Two methods may be adopted for the determination of the inequalities of the first and second order in terms of the masses.

"The method of *interpolation* has this advantage; the actual work of the greater part of the investigation may be given to a computer; the numerous checks which present themselves preventing the possibility of any serious error escaping notice. On the other hand, this method has this inconvenience, that the computation being possible only for numerical data, everything has to be recommenced as soon as these data have undergone any considerable change depending upon the secular variations of the elements of the orbits.

"The development of the perturbing functions, as well as that of the inequalities of the elements under an algebraic form, in which the terms which vary with the time are left indeterminate, leads, on the contrary, to expressions which will serve for all succeeding ages. This is the course which it appeared to me necessary to follow in order to render a really important service to astronomy.

"Consequently, in the expressions I have adopted, the excentricities, the inclinations, and the longitudes of the perihelia and nodes, are, like the mean longitude, left in the form of variable quantities. The mean portions of the major axes are alone treated as given numbers, as is quite allowable, since these undergo no secular variation.

"The first chapter is devoted to the development of the transcendentals on which the determination of the coefficients depends.

"In the second chapter we give the algebraic development of all the terms of the perturbing functions which occur in the theory of *Jupiter* and *Saturn*.

"The terms of the great inequality depending on five times the mean motion of *Saturn*, less twice that of *Jupiter*, are developed with care to the seventh order.

"The third chapter contains the numerical expressions peculiar to *Jupiter* and *Saturn*.

"In the fourth chapter the perturbations of *Jupiter* are determined to the first order in relation to the mass of *Saturn*. For example, for the part of the great inequality depending on the angle

$$5\,l' - 2\,\lambda - \varpi' - 2\,\omega$$

the following expression is found:—

$$-(1+\mu')\begin{pmatrix} 791''\cdot32\,\beta^2\beta' - 2''\cdot00\,\beta^4\beta' - 15''\cdot06\,\beta^2\beta'^3 - 1''\cdot25\,\beta^2\beta'\,\varkappa^2 \\ +\,0''\cdot07\,\beta^4\beta'^3 + \;0''\cdot12\,\beta^2\beta'^5 \end{pmatrix} \sin.\begin{pmatrix} 5\,l' - 2\,\lambda \\ -\varpi' - 2\,\omega \end{pmatrix}$$

in which $1+\mu'$ represents the real mass of *Saturn*; β and β' are the ratios of the excentricities of *Jupiter* and *Saturn* at any

given epoch, to their values at the fundamental epoch fixed at mean noon on January 1, 1850; $\varkappa$ is the ratio of the mutual inclination of the orbits of *Jupiter* and *Saturn* to the value of this inclination at the fundamental epoch; π' and ω represent the longitudes of the perihelia of *Saturn* and *Jupiter*.

"The fifth chapter contains the inequalities of the elements of *Saturn* in the same form.

"The inequalities of the second order in terms of the masses are sufficiently sensible to make a careful determination of them important; the fifth chapter is devoted to this object. It is necessary to put them in the same form as for those of the first order, so that by combining the whole of them we may have the complete expressions for the periodical variations of the elements in the form in which they may be easily referred to any given epoch.

"On account of the magnitude of the perturbations of the first order, and the smallness of the divisors $2n' - n$, $3n' - n$, $5n' - 2n$, and $10n' - 4n$, a large number of terms of the second order are sensible; they depend on the combination of different groups, and in each of these groups there are a considerable number of terms of which notice must be taken, if we wish to obtain an accurate result.

"For example, the term depending on the angle $5l' - 2\lambda - 2\omega - \pi'$, corresponding to that which we have already given to the first order, depends on not less than 18 combinations of groups, taken two and two, in each of which about 10 combinations of terms occur. The total gives an angle of 17 seconds for the co-efficient.

"The determination of the complete expression of these terms has been brought into a form, which, though leaving it complicated, is one in which we may proceed securely, leaving the variable elements indeterminate as for the first order.

"But how can we feel certain that in the course of such numerous operations no error has been committed?

"In the algebraic form, by attributing to the two planets determinate mean longitudes; for example, by placing the planets at their perihelia or their aphelia, we can obtain simple expressions of the corresponding value of the perturbing function, and thus deduce relations between the co-efficients of the general expression of this function, which form a valuable means of verification. These verifications, carefully made *à posteriori*, have been all found exact. The only exception has been for a very small term of the perturbing function of *Saturn*, arising from the reaction of *Jupiter* upon the *Sun*; a term of a very high order, quite insensible, which had changed its sign.

"In the numerical form, although all the co-efficients of the developments have been three times revised, we wished to determine their sums anew by the method of interpolation, of which we have already spoken; and we have even continued the work far enough to form an independent determination of the question

we are discussing. This second investigation is in complete accordance with the first.

"We may then hope that astronomers will accept with confidence the work which we now place before them. It is our intention to print elsewhere all the details necessary to enable any person to verify the parts which he may be interested in examining more closely. In this manner, should a doubt arise on any point, it will easily be seen in what direction the work of verification should be directed.

"The two succeeding sections comprise the theories of the perturbations of *Jupiter* by *Uranus* and *Neptune*.

"The fourth contains the perturbations of *Saturn* by *Uranus* and *Neptune*.

"Then follow three sections, containing the perturbations of *Uranus* by *Jupiter*, *Saturn*, and *Neptune*. The investigation of the perturbations of *Uranus* by *Saturn* was made many years ago, but I then followed the method of interpolation. In the present work, in which the results agree with the former one, I have adopted the algebraic form, which can be applied to any epoch.

"The last sections contain the perturbations of *Neptune*, produced by *Jupiter*, *Saturn*, and *Uranus*.

"To bring this work to a satisfactory conclusion, it will be necessary,—

"1. To calculate the formulæ, and to reduce them into the form of provisional tables.

"2. To combine all the accurate observations of the four planets, and to re-discuss them in order to refer their positions to the same system of co-ordinates.

"3. By means of the provisional tables to calculate the apparent places of the planets for the epochs of the observations.

"4. To compare the observed with the tabular places; to deduce the corrections of the elliptic elements of the four planets, and to determine if the agreement is then perfect.

"5. Should the agreement not be satisfactory, to investigate the cause of the discordance."

Observations of the Planet Venus, with a 6-inch Silvered Glass Reflector. By R. Langdon, Esq.

(*Communicated by J. Norman Lockyer, F.R.S.*)

On May 1st, 1871, I had a good view of the planet *Venus;* but I could not at first see her to my satisfaction as her light was so bright. She had more the appearance of a miniature sun than a star; but I put a diaphragm of blackened card in the eye-piece, and made a small hole through its centre with a piece of hot wire. I found this arrangement keep out to a great extent the glaring rays. I also sometimes used a slip of slightly-tinted glass in

front of the eye lens; this enabled me to bring the planet entirely under subjection. Her shape was that of the Moon when a little more than half full. I distinctly saw a dull, cloudy-looking mark along her bright limb, curving round parallel to it, extending nearly across the disc, each end terminating in a point; joining this at the eastern extremity, was another and darker mark of a club shape, its small end joining the point of the mark previously described. (See Sketch No. 1.) I watched these marks for half-an-hour; I saw some marks again the next evening, but before I could examine them the planet was hid behind some clouds.

On May 6th, at 7.45 P.M., there was a cloud-like mark extending straight across the disc, and a club-shaped mark nearly in the centre, with its small end nearly touching the straight cloud. On the western limb another dark mark had made its appearance; it was not quite so large as the other, and it was not club-shaped; but its sides were parallel to each other till they approached the

straight cloud, when they appeared to divide, each side curving round away from the other. (See Sketch No. 2.)

I took much interest in watching these spots, as I had read that "it was very doubtful whether any marks had ever been seen on this planet." I called several men to look at them, and they were able to describe them, although they had no previous knowledge or idea of what they were likely to see. One man was very confident it was the Moon he was looking at, but when I pointed out to him the Moon was not in the neighbourhood, he said he thought it was the Moon, because he could plainly see the dark patches on its surface.

On May 13th, at 7.30 P.M., there was a dark mark of a pear shape, extending from near the western edge to two-thirds the distance across the bright disc. This mark was not so dark as

those seen on the 1st and 6th, but it was much larger. (See Sketch No. 3.)

On July 28th, at 8 P.M., there were visible five dusky marks along the planet's terminator, and one nearly in the centre of the crescent; but they were not so well defined as those before described; but what seemed to me more remarkable, was that the southern horn was rounded off considerably, whilst the northern horn was quite sharp, and ran out to a very fine thread-like point. (See Sketch No. 4.)

On October 13th, at 5.45 A.M., I saw *Venus* as a beautiful little crescent. She was well defined, and both horns were as sharp as the finest-pointed needles. I think I detected a dusky cloud-like mark about half way from the centre to the northern horn; but I am not quite sure about this, as I had to leave the telescope before I could complete my sketch. (See Sketch No. 5.)

On October 25th, at 8.10 A.M. On this occasion I was gratified with a sight which I had waited for and longed to see for many years; that was to have a good view of *Venus* by daylight. I now had the longed-for opportunity, and it turned out as I expected it would—that the superior light of the Sun would overcome that of the planet to such an extent as would enable me to see her better than I had ever seen her before. I could now plainly perceive the jagged nature of the terminator, the unevenness of which could not be mistaken; but what was very remarkable, the northern horn was bent in towards the centre of the planet; it appeared as if a notch had been cut in the inside, and a slice cut off from the outside. (See Sketch No. 6.)

I have no idea what was the cause of this appearance; I have never seen it so before, neither do I recollect ever having read of such a phenomenon. I did not perceive any markings on this occasion, but there was a kind of haziness along the whole length of the terminator; but I considered this at the time to have belonged to the terminator rather than to any markings on the disc. The terminator on this occasion was inky black.

November 9th, I saw *Venus* every half-hour during the day up to 1 o'clock. I made a sketch at 12.20 P.M. I could now distinctly see the jagged terminator, the nature of which was so much like that of the Moon as it was possible to conceive, except that if we compare the Moon's terminator to a piece of network, that of *Venus* would be represented by a piece of fine lace. I could also see some thin, cloudy marks on her disc. The southern horn was very sharp; the northern one was a trifle rounded.

I saw *Venus* on February 5th, 1870 (a few days before her inferior conjunction with the Sun), the bright part was an exceedingly beautiful fine crescent; but myself and several other people could see the whole body of the planet in the same manner as we see the dark limb of the Moon when *Earth-shine* is falling upon it; but I did not make any sketch at the time.

I have observed *Venus* a great many times besides those mentioned above, having made it my special work to do so, and

have on several occasions strongly suspected markings to have been visible; but I have not mentioned them, and have only described those times upon which I have had no doubt of what I had seen.

Silverton Station, near Cullompton, Devon.

New Tables of Uranus. By Simon Newcomb.

(*Extract from a Letter addressed to the Astronomer Royal.*)

"When I last enjoyed the pleasure of writing to you, I made known that I had long been engaged in perfecting the theory and tables of *Uranus*. After devoting a large amount of labour to this subject during the last twelve years, the work is now, I hope, approaching its close. I have fixed upon the opposition of 1871–2, as the last one on the observations of which the tables are to be founded.

"I now write to inquire whether you will kindly communicate to me the results of the Greenwich Observations of *Uranus* during the years 1870, 1871, and 1872, to be used in the final equations of condition.

"So far as I have yet compared my final provisional theory with observations, the indications are, that the motion of the planet during the ninety years since its discovery, will be represented within a small fraction of a second of arc, and, in consequence, that no evidence will be found to indicate the action of a trans-Neptunian planet. On this point, however, I cannot yet speak positively. So far as I know, the planet *Neptune* has not yet deviated sensibly from my tables. With the comparison of the Greenwich Observations for 1871 with the *Nautical Almanac*, you are better informed on this point than I am.

"Permit me to congratulate you on setting at rest the question whether the thickness of the objective has any influence on aberration. There could be little doubt of the result after Hoek's experiments; but it is always more satisfactory to test a theory by a crucial experiment—*a posteriori*."

Washington, May 16th, 1872.

Discovery of Minor Planet (121). By Mr. Watson.

(*Extract from a Letter addressed to the Astronomer Royal.*)

I have the pleasure to send you the following observations of a new planet which I have discovered:—

Ann Arbor M.T.		R.A.	Decl.	No. of Comparisons.
	h m s	h m s	° ′ ″	
1872 May 12	12 5 0	16 20 41·20	−18 53 6	1
12	14 13 22	16 20 37·58	−18 53 9·4	5
13	11 13 22	16 19 59·33	−18 52 46·1	6

It shines like a star of the 11th magnitude.

Ann Arbor, May 14, 1872.

Observations of Occultations of Stars by the Moon (with the deduced Equations between the Errors of the Lunar Elements); and of Phenomena of Jupiter's Satellites; made at the Radcliffe Observatory, Oxford, in 1871 *and* 1872.

(*Communicated by the Radcliffe Observer.*)

Occultations.

No.	Day of Observation.	Phenomenon.	Moon's Limb.	Oxford Mean Solar Time. h m s	
	1871.				
1	Oct. 23	Dis. of τ^1 Aquarii	Dark	8 32 28·0	M.
2	,,	Dis. of τ^2 Aquarii	Dark	9 42 43·1	M. & L.
3	,,	Reapp. of τ^2 Aquarii	Bright	10 53 20·9	M.
4	Nov. 15	Dis. of λ Sagittarii	Dark	5 9 23·4	M. & K.
5	16	Dis. of h^2 Sagittarii	Dark	6 55 46·4	,,
6	18	Dis. of ε Capricorni	Dark	6 57 55·0	,,
7	27	Dis. of ι Tauri	Bright	10 3 20·8	M.
8	,,	Reapp. of ι Tauri	Dark	11 18 23·4	,,
9	Dec. 1	Dis. of γ Cancri	Bright	18 0 58·4	,,
10	,,	Reapp. of γ Cancri	Dark	19 13 9·0	M. & K.
11	20	Dis. of ν Piscium	Dark	12 44 43·9	L. & K.
12	,,	Reapp. of ν Piscium	Bright	13 17 3·6	K.
	1872.				
13	Jan. 23	Dis. of ω Geminorum	Imperfect	6 3 11·4	L.
14	Apr. 13	Reapp. of 5 Geminorum	Bright	7 59 22·5	K.
15	18	Dis. of B.A.C. 3579	Dark	7 51 48·4	M.
16	May 22	Reapp. of ω^2 Scorpii	Bright	10 53 7·6	,,

Oct. 23. All these observations good, and the phenomena instantaneous; the sky was rather cloudy.

Nov. 15. The two observers give the same tenth of a second; the disappearance was very sudden at the beat of the Heliometer clock. No projection on the obscure disk of the Moon, which was distinctly visible. [M.]

Nov. 16. Good; the mean of the observed seconds, 46·0 and 46·8, is taken. The altitude of the star was 2° 21′.

Nov. 18. Good; the star vanished suddenly exactly at the edge of the obscure disk, which was visible; the mean of the observed seconds, 54·9 and 55·1, is taken.

Nov. 27. Disappearance of *ι Tauri*; the observation pretty good, though the disk was considerably obscured by clouds. For the reappearance K gave $24^{s}\cdot3$.

Dec. 1. Disappearance of *γ Cancri;* pretty good. The reappearance was instantaneous. [M.] K gave the same tenth very nearly.

Dec. 20. The two observers agree within a tenth. The Moon tremulous at reappearance.

1872, April 18. Instantaneous.

May 22. The Moon slightly eclipsed, and exactly full; the observation very good. The other observations of this evening were prevented by trees, which were in the way of the Heliometer.

In the following table of the errors of lunar elements resulting from the occultations the Greenwich notation is used, and the ele-

ments of the *Nautical Almanac* uncorrected. All the computations have been made by Mr. Main by the method given in his treatise on *Spherical and Practical Astronomy*.

The observations are referred to by the Nos. of reference given above.

1 $+ 0''.93 = + 0.815 \times e + 0.543 \times f - 0.815 \times x - 0.544 \times y - 0.203 \times t - 1.720 \times m - 0.942 \times n.$

2 $+ 7.39 = + 0.921 \times e - 0.322 \times f - 0.921 \times x + 0.321 \times y - 0.423 \times t + 1.592 \times m - 0.942 \times n.$

3 $- 7.08 = - 0.747 \times e + 0.641 \times f + 0.747 \times x - 0.641 \times y + 0.444 \times t - 2.884 \times m - 0.941 \times n.$

4 $+ 7.53 = + 0.902 \times e + 0.132 \times f - 0.902 \times x - 0.134 \times y - 0.497 \times t + 0.892 \times m - 0.991 \times n.$

5 $+ 9.21 = + 0.845 \times e - 0.376 \times f - 0.845 \times x + 0.374 \times y - 0.496 \times t + 2.718 \times m - 0.982 \times n.$

6 $+ 3.97 = + 0.212 \times e - 0.975 \times f - 0.212 \times x + 0.975 \times y - 0.268 \times t + 3.366 \times m - 0.960 \times n.$

7 $+ 0.91 = + 0.927 \times e - 0.035 \times f - 0.927 \times x + 0.037 \times y - 0.377 \times t - 1.140 \times m - 0.887 \times n.$

8 $- 11.63 = - 0.941 \times e + 0.593 \times f + 0.941 \times x - 0.592 \times y + 0.447 \times t - 0.362 \times m - 0.887 \times n.$

9 $+ 9.66 = + 0.922 \times e - 0.078 \times f - 0.922 \times x + 0.080 \times y - 0.375 \times t + 0.929 \times m - 0.890 \times n.$

10 $- 9.75 = - 0.775 \times e - 0.545 \times f + 0.775 \times x + 0.546 \times y + 0.405 \times t - 0.276 \times m - 0.890 \times n.$

11 $+ 3.94 = + 0.116 \times e - 0.993 \times f - 0.116 \times x + 0.993 \times y - 0.251 \times t + 2.771 \times m - 0.910 \times n.$

12 $- 10.64 = - 0.828 \times e - 0.556 \times f + 0.828 \times x + 0.556 \times y + 0.270 \times t - 0.268 \times m - 0.910 \times n.$

13 $+ 2.66 = + 0.427 \times e - 0.882 \times f - 0.427 \times x + 0.883 \times y - 0.258 \times t + 0.895 \times m - 0.882 \times n.$

14 $- 9.21 = - 0.856 \times e + 0.330 \times f + 0.856 \times x - 0.327 \times y + 0.380 \times t - 2.083 \times m - 0.891 \times n.$

15 $+ 5.23 = + 0.711 \times e + 0.675 \times f - 0.711 \times x - 0.674 \times y - 0.366 \times t - 1.611 \times m - 0.901 \times n.$

16 $- 7.19 = - 0.933 \times e - 0.140 \times f + 0.933 \times x + 0.139 \times y + 0.455 \times t + 1.079 \times m - 0.983 \times n.$

Phenomena of Jupiter's Satellites.

Day of Obs. 1872.	Satellite.	Phenomena.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Jan. 3	II.	Occ. reap. last contact	9 54 3·4	9 59	K.
8	I.	Ecl. dis. last seen	7 6 18·7	7 10 33·1	„
15	I.	Occ. reap. first app.	11 15 14·0	11 21	„
	I.	„ bisection	11 16 23·8		
	I.	„ last contact	11 17 43·6		
16	III.	Ecl. reap. first seen	11 58 54·9	12 1 47·2	L.
23	I.	Tr. ing. first contact	7 54 1·2	8 4	„
	I.	„ last contact	8 2 4·9		
31	I.	Occ. dis. first contact	6 43 47·9	6 55	„
	I.	„ last contact	6 51 36·6		
	I.	Ecl. reap. first seen	9 31 33·3	9 36 42·7	„
Feb. 2	II.	Tr. ingr. first contact	9 58 13·6	10 6	„
	II.	„ last contact	10 5 22·4		
8	I.	Tr. egr. first app.	8 13 17·9	8 19	K.
	I.	„ bisection	8 14 32·7		
	I.	„ last contact	8 16 2·4		
10	IV.	Ecl. reap. first seen	9 42 29·4	9 50 38·5	„
	IV.	„ usual brightness	9 48 45·4		
17	III.	Tr. ingr. first contact	11 18 13·3	11 26	„
	III.	„ bisection	11 22 12·7		
	III.	„ last contact	11 26 12·1		
20	II.	Tr. egr. last contact	6 43 43·7	6 46	L.
21	I.	Occ. dis. first contact	12 5 44·7	12 13	K.
	I.	„ last contact	12 10 14·0		
23	I.	Occ. dis. first contact	6 31 37·7	6 39	L.
	I.	„ bisection	6 34 47·2		
	I.	„ last contact	6 36 46·9		
Mar. 1	I.	Ecl. reap. first seen	11 39 54·3	11 45 2·0	„
5	II.	Tr. ingr. first contact	8 30 1·0	8 38	„
	II.	„ last contact	8 36 20·0		
	II.	Sh. ingr.	10 41 47·9	10 44	„
	II.	Tr. egr. first app.	11 23 5·1	11 33	„
	II.	„ last contact	11 30 4·0		

Jan. 3. Cloudy.

Jan. 8. The satellite disappeared very close to the edge of the planet, and became very faint before disappearing.

Jan. 16. Overcast, but the observation is satisfactory.

Feb. 21. Very faint; the planet tremulous and ill defined; light clouds.

Day of Obs. 1872.	Satellite.	Phenomena.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Mar. 6	III.	Occ. dis. first contact	8 6 51·7		
	III.	„ bisection	8 10 36·1	8 16	K.
	III.	„ last contact	8 14 35·4		
	III.	Occ. reap. first app.	11 36 10·6		
	III.	„ last contact	11 43 39·4	11 44	„
	III.	Ecl. dis. last seen	12 32 34·9	12 38 12·4	L.
9	I.	Tr. ingr. first contact	7 30 15·0		
	I.	„ bisection	7 32 44·6	7 37	K.
	I.	„ last contact	7 35 29·1		
	I.	Tr. egr. first app.	9 49 23·1		
	I.	„ bisection	9 51 37·7	9 56	„
	I.	„ last contact	9 54 7·3		
10	I.	Ecl. reap. first seen	8 3 53·5	8 9 7·5	L.
12	II.	Tr. ingr. first contact	10 56 59·5		
	II.	„ bisection	10 59 59·0	11 5	„
	II.	„ last contact	11 4 18·3		
13	III.	Occ. dis. first contact	11 47 37·1	11 55	K.
16	I.	Tr. ingr. first contact	9 20 59·5		
	I.	„ bisection	9 23 59·0	9 27	„
	I.	„ last contact	9 26 28·6		
	I.	Sh. ingr. first app.	10 35 44·6		
	I.	„ well on disk	10 38 4·3	10 38	L. & K.
Apr. 6	II.	Tr. egr. first app.	10 45 20·4		
	II.	„ bisection	10 47 35·0	10 54	K.
	II.	„ last contact	10 50 4·6		
8	I.	Tr. ingr. first contact	9 28 13·9		
	I.	„ bisection	9 30 58·4	9 35	„
	I.	„ last contact	9 33 43·0		
	I.	Tr. egr. first app.	11 50 20·8		
	I.	„ clear of disk	11 56 19·8	11 54	„
	IV.	Tr. egr. bisection	12 14 16·9		
	IV.	„ last contact	12 20 15·9	11 56	„
9	I.	Ecl. reap. first seen	10 14 26·0		
	I.	„ full brightness	10 15 36·9	10 19 34·8	L.

Mar. 6. Moisture on the object-glass, which rendered the eclipse of the third satellite less satisfactory than it would otherwise have been. It was about 40° in entering the shadow of the planet.

Mar. 13. Cloudy.

Mar. 16. Cloudy at the ingress of the first satellite.

Apr. 8. The observations of the egress of the first satellite not very good, as *Jupiter* was seen through the branches of a tree. The " bisection " of fourth satellite not quite so satisfactory as the last contact, the satellite being very faint, and the planet seen through the branches of a tree.

Day of Obs. 1872.	Satellite.	Phenomena.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Apr. 11	III.	Ecl. reap. first seen	12 0 52·0	12 4 53·5	K.
	III.	„ usual brightness	12 3 5·6		
13	II.	Tr. ingr first contact	10 26 46·3	10 37	L.
	II.	„ last contact	10 34 15·1		
15	II.	Ecl. reap. first seen	10 55 40·9	11 1 5·9	K.
	II.	„ full brightness	10 56 43·2		
	I.	Tr. ingr. first contact	11 22 6·1	11 30	„
	I.	„ bisection	11 24 35·7		
	I.	„ last contact	11 27 35·2		
16	I.	Occ. dis. first contact	8 30 0·2	8 40	L.
	I.	„ last contact	8 36 9·2		
17	IV.	Ecl. reap. first seen	10 14 18·6	10 22 32·6	„
	IV.	„ fully seen	10 19 27·8		
	IV.	„ full brightness	10 22 27·3		
23	I.	Occ. dis. first contact	10 28 48·1	10 37	„
	I.	„ bisection	10 31 47·6		
	I.	„ last contact	10 34 47·1		
May 1	I.	Tr. ingr. first contact	9 45 52·6	9 52	K.
	I.	„ bisection	9 48 37·1		
	I.	„ last contact	9 51 51·6		

Apr. 15. The same remark applies to the ingress of I. as on April 8.

The initials M., L., and K., are those of Mr. Main, Mr. Lucas, and Mr. Keating.

The observations were generally made either with the Heliometer or with the 10-foot telescope, or with both. The 42-inch Dollond was used on November 15 and 16 by K. and on December 20 by L., for the occultations of stars.

On Photographic Irradiation in over-exposed Plates. By Lord Lindsay and Mr. A. Cowper Ranyard.

The most cursory observer of any of the recent corona photographs must have remarked the apparent eating-in of the prominences over the limb of the dark Moon. A more careful examination of the photographs shows that the whole limb of the Moon is more or less eaten into, and that the indentations under the prominences are only exaggerations of a phenomenon which is present at all parts of the limb, but which varies in intensity according as the dark limb of the Moon is projected on a brighter or less luminous background.

In all over-exposed photographs of luminous objects upon a dark background, the brighter parts of the picture are found to be surrounded by a nebulous haze or border of light, which

increases the diameter of the image formed by the luminous objects at the expense of those which are less luminous.

This nebulous haze has often been spoken of as "the extension of the chemical action," but without begging the question of its cause, we propose to speak of it as photographic irradiation. It has been found to vary with the time of exposure, and the relative brightness of the object and its background.

On examining the effects of photographic irradiation in a decidedly over-exposed picture, it will be seen that the nebulous fringes round luminous objects are distinctly divided into two parts—an inner and very marked border of light, following the contour of the luminous objects, and an outer and much less definite haze, thus:—

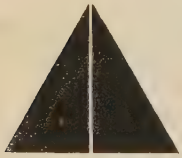


Fig. A.

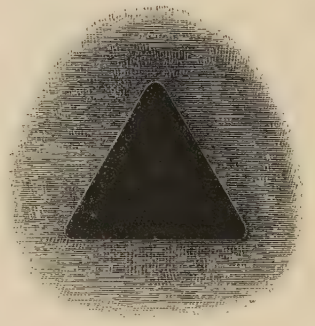


Fig. B.

where fig. A represents a normal photograph, and fig. B a decidedly over-exposed plate from the same object.

The inner border of light fades gradually from the inside outwards, and it is very difficult, and indeed impossible, to tell where the true image of the luminous object ends, and its photographic irradiation begins. While, on the other hand, the boundary between the outer and inner fringes (or halos) of irradiation is more definitely marked, although it would be difficult to say with any absolute precision, at what point the inner fringe terminates.

Our first experiments were devised in order to test whether reflections from the back surface of the plate played any part in the production of the fringes; for this purpose plates of ebonite and the so-called non-actinic yellow glass were prepared.

In the over-exposed photographs taken on ebonite, it was found that the outer haze had entirely disappeared; while in the photographs taken on plates of yellow glass the outer haze is still distinctly to be traced, though it is much fainter than on an ordinary white glass plate with the same exposure.

By placing a piece of wetted black paper at the back of an unground plate the outer haze may be greatly reduced, while it was found that by grinding both the back and the front surfaces

of a yellow glass-plate, and covering the back with a coating of black varnish, the outer haze may be rendered quite imperceptible, while, however, the inner border of irradiation still remains as before.

From these experiments we may conclude that the outer haze is produced by reflections from the back of the plate; and the action of the wetted black paper in reducing the outer irradiation, may be explained by the consideration that the change of refractive index in passing from the glass to the film of water behind, is much less than in passing from glass into air. There is consequently less reflection at the back surface of the plate; most of the light immerges into the film of water, and is then absorbed by the black paper.

Fig. C represents a photograph taken upon a yellow glass plate, with a backing of wet black paper, but otherwise exposed under similar conditions to the photograph represented in fig. B.

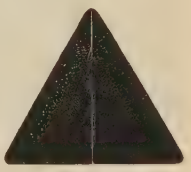


Fig. c.

The outer irradiation halo may therefore be entirely avoided for the future in any corona or other necessarily over-exposed photographs by the use of the opaque plates. If, however, it is considered important that the negatives should be capable of being copied by transmitted light, the outer halo may be still to a great extent avoided by the use of yellow glass plates with a backing of wet black paper or black varnish.

Secondly, as to the inner and more definitely-marked irradiation-edge which remained and seemed to be unaffected by the precautions that had served to rid us of the outer halo. Since the inner fringe was equally to be found on an opaque, and on a transparent plate, we felt ourselves justified in seeking for its cause in front of the first or upper surface of the prepared plate; that is, it must be referred either to some action taking place within the thickness of the collodion, or to the optical imperfections of the instrument.

In order to determine whether the scene of action lay within the thickness of the collodion, we placed an ivory ruler with a bevelled edge in immediate contact with the collodion film. The plate with the ruler upon it was then exposed within the camera, so that the image of an incandescent platinum wire fell partly upon the collodion film, and partly upon the ivory ruler. If the scene of action lay within the collodion film, we might expect the inner irradiation fringe to extend itself under the edge of the ruler, while if it were due to the optical imperfections of the lens, the image of the wire would be cut off sharply by the edge of the ruler.

On removing the plate from the camera, and before the ruler was shifted from its place on the collodion, the whole was exposed for a few seconds to the action of the light from a gas-burner, in order that the position occupied by the edge of the

ruler might be faintly printed upon the collodion film. On developing the plate, it was found that the image of the wire was sharply cut off at the place occupied by the edge of the ruler, as in fig. D.

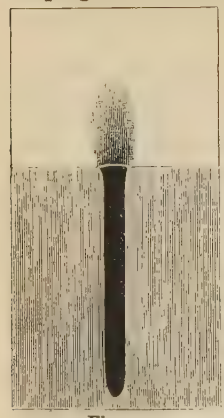


Fig. D.

The very faint action extending inwards under the ruler being evidently due to the want of perfect opacity in the ivory, it seems, therefore, to be clearly proved that the inner irradiation-edge is not caused by any chemical or other action taking place within the thickness of the collodion. But must be referred to the optical imperfections of the instrument which throws the image upon the collodion film.

It is instructive to remark that the photographic image of the wire is not cut off by an absolutely straight line at the edge of the ruler, but it is slightly convex, and is separated from the faint action which has apparently taken place through the ruler by a very narrow bright line, which appears to indicate the presence of a small capillary film of liquid along the edges of the ruler forming a minute cylindrical lens. At the point where the collodion was acted upon by the light, the minute cylindrical lens appears to have been interfered with, and depressed inwards towards the ruler, we may therefore conclude that the collodion film is slightly swelled or thickened by the action of the light upon it.*

The cause of the inner irradiation-edge seems to be that every point of a luminous object is not represented by a simple point of light in the luminous image ; in other words, the circle of least diffusion of any pencil is a curve of sensible area, of which the central and most intense portions imprint themselves first upon the collodion.

Some further experiments were made in order to test whether the size of the circles of least diffusion was chiefly owing to chromatic aberration (in which case the difficulty might be got rid of by the use of reflectors) ; for this purpose a bath of solution of sulphate of copper was placed in front of a gas-burner, and the triangular diaphragm shown in fig. A was then placed

* We are at present unable to find any explanation of the slight apparent thickening of the end of the image of the wire where it abuts upon the ruler, but the same thickening is to be found in all the plates. It may be well to remark that it appears evident from slight indication in the negatives which it would be difficult to render in a woodcut, that the true edge of the ruler coincides with the inner side of the white line (or with the side away from the image of the wire). The convexity of the end of the image of the wire cannot, therefore, be regarded as indicating even a slight chemical encroachment. The slightly tapered appearance of the other end of the image of the wire is due to the fact that the platinum incandescent wire is cooled at its points of contact with the thick copper wires of the circuit.

between the sulphate of copper bath and the lens of the camera, but the blue screen thus formed seemed to have very little effect in altering the breadth of the irradiation-fringe, only slightly retarding the rate of its formation; a similar result was obtained on placing a piece of yellow glass in front of the diaphragm; in this case, however, the formation of the fringe was still further retarded.

Photographers have long known that by making use of stops they can obtain a much sharper image. By way of experiment, we cut off the edges of the lens with a circular stop, and found that the inner irradiation fringe was thus greatly decreased. It seems, therefore, fair to argue that the aberration of oblique pencils exceeds in magnitude the other disturbing causes, and that it will be well, in making preparations for the photographic observation of the Transit of *Venus*, to avoid as much as possible all oblique pencils.

We would, therefore, place our photographic plates in the primary focus, and thus avoid the necessarily deep curves of any arrangement of lenses which may be used for enlarging the image. Whether it would be best to make use of a reflector or a refractor, remains to be settled by further experiment, but our present experiences would lead us to vote in favour of the reflector.

We cannot conclude without returning our best thanks to Mr. H. Davis, who has rendered us the most willing assistance in carrying out all of the foregoing experiments.

The Aurora of February 4th, 1872. By the Rev. J. Slatter.

A desire having been expressed by Mr. Finlayson in the April Number of the *Monthly Notices* for any observation of this aurora, I forward to you the entry I made of it.

Feb. 4, $7^h\ 20^m$. A fine aurora with much rosy light, but hidden by low fleecy clouds, with a slight drizzling rain. At the time indicated, there was a break in the clouds which showed the dome-formation, and suggested a corona either formed and passed, or about to form, exactly over *Aldebaran*, by about 5°, which was then in azimuth almost on the magnetic meridian.

My place of observation is in N. lat. 51° 30′ 15″; long. W. 1° 1′ 15″.

On Oct. 24, 1870, I obtained a simultaneous observation of the corona with Greenwich, viz. at $8^h\ 20^m$, G.M.T. At this station it exactly covered *γ Pegasi;* at Greenwich it covered a spot midway between *β* and *η Pegasi*, which may be assumed to have been R.A. $22^h\ 47^m\ 13^s$, N.P.D. 61° 32′.

From these data it would appear that the height of the corona was nearly 118 miles in the zenith of a spot in the Channel about thirty miles north of Havre. But this depends entirely on the assumption that it was the same phenomenon (*i.e.* identical and not only apparent) which was observed at both places. The

only other observation I ever made, in which I was satisfied by ocular demonstration of the identity of the phenomenon, was the height of an auroral arch with a base of nineteen miles which gave a height of only 15,000 feet. With regard to the intensity of the light on the occasion just referred to, viz. Oct. 24, 1870, it may be worth recording that, great as was the amount of light pervading the atmosphere, no portion of it, not even the corona, was capable of being seen by reflexion in the centre of the river.

Streatley Vicarage, June 4th, 1872.

Observations of the Solar Prominences, from Jan. 1 to April 29. By Father Secchi, Director of the Roman Observatory.

Father Secchi communicates to the Paris Academy his observations of the prominences during the four solar rotations ending April 29, thus completing, with the observations already recorded (see *Monthly Notices* for March), an entire year of observations. In this period there were fifty-nine days of complete observations.

During this interval, the law was confirmed, which gave a maximum of prominences in the region of spots, and a minimum of the equator. But the relative maximum in the polar zones was scarcely sensible.

"The general absence of polar prominences," proceeds Secchi, "during the interval was remarkable." They were replaced by sensible elevations of the chromatosphere. Counting only as prominences those whose height reached or surpassed forty seconds, prominences were found to be very rare.

"The aspect of the granulations, and of the brighter bands around the polar zones, corresponded with this absence of polar prominences; as the former could scarcely be recognised, while last year they were very visible.

"The intensity and number of the faculæ are also diminished.

"Dividing the prominences into three classes, according to their direction with respect to the poles, the following numbers are obtained:—

Indifferent	398
Directed towards the poles.. ..	342
Directed towards the equator ..	67
Total	807

"In the observations, especially the later ones, great attention has been paid to the direction of the threads or hairs of the chromatosphere, and the results obtained have been the following:—

"In general, in mean latitudes, the threads are also directed towards the poles, but there are numerous exceptions, especially in the neighbourhood of prominences, spots, and granulations. At the equator and poles, there are no constant rules.

"The variability of this inclination is often astonishing at one and the same place. It even appears to change during the observation, in such sort that I have often been the victim of an illusion; but a sustained attention, in the clearer days, has proved that this variability is real. One would be tempted to compare this diffusion to a species of very variable electric sparkling, rather than to an emission of real matter: but it would be premature to pronounce, and we do not pretend here to prejudge the true interpretation. It is necessary to mention that sometimes, when the hairs are very fine, they appear divided into two opposite directions, with respect to the slice of the chromatosphere limited by the border of the slit. All the circumstances show how difficult these observations are, and how much yet remains to be cleared up.

"One of the most remarkable peculiarities attracting attention in these researches, is the fixity of the chromatospheric layer and its features, amidst the apparent agitation of the solar edge. It is truly astonishing to see the streak adorned with fringings and hairs of the utmost delicacy, resting immovable in the middle of the disturbance agitating the light of the Sun's limb. This proves that the agitation is factitious, that it is due to our atmosphere, and does not displace the direct image of the Sun. The effect is somewhat like that produced during the observation of stars near the horizon, where one sees the black lines remaining immovable in the midst of the incessant waves seen to traverse the spectrum. This leads me to the inquiry whether one would not gain considerably in precision in taking the passage of the solar disk with the help of a spectroscope: observations have shown me that there would be a sensible advantage in this course; but to render this system perfectly practicable, it would be necessary to fulfil certain conditions as to details, which I have not yet been able to accomplish.

"I have inquired whether the direction of the hairs of the chromatosphere might not be influenced by this agitation, but I have convinced myself that it is not so influenced to any extent. These hairs, which seem to us so fine, are veritable flames on the Sun, and it is natural that our atmosphere should have no influence upon them.

"It is seen, by all these results, how necessary it is that the observations of the prominences should be made in a continuous manner; and at least during one full solar spot period, in order that we may be enabled to compare the vicissitudes of the chromatosphere with those of the photosphere. The three last months have shown a medium activity of spots and prominences, in reality they seem to be reviving. I occupy myself, at present, in discussing the relations of the two phenomena, availing myself of the drawings which I have made during the year. The comparison of these drawings with the fine photographs of M. Capello, has convinced me that our drawings, without attaining the perfection

of photographic images, are nevertheless capable of being sufficiently useful to the science of the day.

"When the sky is not clear, the chromatosphere can still be observed, if the veil is simply caused by mists formed of water globules; one can see nothing if the veil is formed of small ice-crystals. With the aqueous veil, the bright line of the chromatosphere seems bordered by two very strong dark lines, which seem to detach it from the Sun; these lines are evidently due to the aqueous vapour in our atmosphere. The fact that the length of these lines increases with the density of the veil leaves no doubt as to their origin.

"Let me be permitted to return to the structure of the corona of which I spoke in my last communication, to remove objections which may be raised.

"The curvilinear rays lead us to think that there is a certain circulation there; but ought we to suppose this circulation to proceed like that produced on a calm planet warmed from outside? Not at all, unquestionably. We have in the Sun an element which deserves special recognition; it is not merely the ascensional motion of masses warmed below, but another cause, perhaps more potent, the material projection from within outwards, which one must also consider. Neglecting the resistance of the medium in which the solar emissions take place, we should have, taking the evaluations of most distinguished men of science, initial velocities capable of projecting matter beyond the sphere of solar attraction. No doubt these velocities are exceptional, and are attained only during the epochs of greatest activity. But if the ordinary velocities cannot project matter to such distances, they may well carry it as far as the limits of the visible corona, after which it would fall back upon the Sun, after having been cooled by dilatation and by radiation in space These masses, in rising, feel the influence of the layers they traverse, which should modify their direction, and bend them into curvilinear threads if these layers themselves have a circulation of their own.

"According to these considerations, the solar corona would not be merely formed of a layer circulating quietly, but it would be broken by jets proceeding from the body of the Sun; its constitution could not be assimilated to that of an envelope in dynamical equilibrium, circulating according to the laws of rotation and of the difference of densities produced by lower heat.

"These considerations seem to me well fitted to enable us to appreciate and explain the movements and the curious curves which are presented in Lord Lindsay's photographs, as well as in the drawings by M. Liais and other observers."

Father Secchi's paper is accompanied by a series of tables resembling those which accompanied his former paper. As the evidence they afford accords in much the same way with his inferences, it does not seem necessary to reproduce these tables.

JOHN BROWNING, DEL.